(2)

A M James

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2nd April, 1958.

Dear Professor Lederberg,

Thank you for your letter of 24th February. Please accept my apologies for not replying earlier, but I have been giving the matter serious thought and discussing it with my associates.

I realise fully that much work has been published in support of the modification theory rather than a process of natural selection. I presume that some of the work to which you refer is that of Hinshelwood and his Oxford School. As I received my early training in that school I suppose I am slightly prejudiced!

Your interpretation of the data in Fig.8 are correct, namely that during the lag phase (up to about 1800 minutes) we never detected any cells with low mobility values. As soon as cell division occurred then the newly formed cells had the low mobility value. The cells with the high mobility values represent the fraction which was dead at the end of the lag phase (we carried out viability measurements during the lag phase). This proportion decreases during the logarithmic growth phase, and in some instances we detected 1 or 2 cells with high mobility values in the stationary phase. I should point out that the cells were stained with crystal violet which could not be washed off, the amount, however, was so small that this did not cause the lowered mobility value. Generally it should be possible to discriminate a minute proportion of organisms with a different mobility to the bulk. In this case it is impossible to make a random selection in the observation chamber since the cells with low mobilities are very small, compared with those of high mobility. This is a very unfortunate situation.

I presume that from the standpoint of the theory of natural selection you would expect 1 cell in 10^8, or thereabouts, in the normal strain to have a low mobility value. I have calculated that to explain the graph (Fig.8) on this theory we should have to postulate 1 organism in 10^{14} with a low mobility value. We have no chance of detecting this. During the 8 years we have been working on this organism we have never recorded such a low mobility value for cells in a normal culture. As far as the theory of natural selection is concerned I should have thought that adaptation to a bacteriostatic agent should occur relatively quickly. Hinshelwood and co-workers find that adaptation to sulphonamides, as measured by decrease of lag and increase in growth rate to normal, is a very slow process. Perhaps I am wrong in this concept.

Recently we have been using your method to obtain protoplasts from Aerobacter aerogenes and so far can report good success. When we have established the

BY AIR MAIL
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AEROGRAMME

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conditions under which they can be stabilized, preferably not sucrose solutions, we hope to make a study of their surface properties and electrophoretic response to various antibacterials and surface active agents.

Yours sincerely,

A. M. James

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